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THE DISTINCTIVE TRAITS OF AN EMPIRICAL METHOD[1]

A METHOD is a repeatable series of repeatable operations aimed at an end. The aim of the method we shall discuss is the augmentation and correction of a body of knowledge.

In the formulation of scientific hypotheses, principles, and systems, there is much room for individual talent and genius and little opportunity for repetitive methods. Psychological rules for it have been proposed, but none of them prescribes a method in an exact sense; at most they are precepts of art. We can not propose, with any rigor, an empirical method of formulating scientific laws, postulates, or hypotheses. But assuming some hypothesis, we can ask for a definitive answer to the question: "What must be the characteristics of propositions and the methods used to test them if they are to deserve the name 'empirical'?"

As a first approximation, in order for a proposition to be amenable to an empirical test or correction, it must imply singular propositions for or against which experience can decide. Implicit in this minimal condition are several concepts requiring examination. We shall investigate three:

(1) the progressive elaboration of the hypothesis to a proposition descriptive of a possible experience;
(2) the obtaining of experiences by which the hypothesis can be confirmed or infirmed; and
(3) the interpretation of these experiences with the purpose of deciding the truth or falsity of the hypothesis.

(1) *The empirical interpretation of hypotheses.* A given hypothesis or principle implies another proposition. Independent evidence for this proposition will not directly confirm the hypothesis, while an evidential denial of the proposition will infirm the hypothesis. Confirmation can come only from infirmation of all its alternative hypotheses through the evidential denial of at least one consequent of each alternative and the absence of denial of any consequent common both to any alternative and to the hypothesis in question. Let us call the hypotheses to be tested in

[1] Read at the meeting of the Eastern Division of the American Philosophical Association at Yale University, December 28, 1946.

any given procedure the *substantive hypotheses*. A substantive hypothesis is empirical if and only if it implies some proposition for or against which some experience can decide. It does not matter whether either the antecedent or the consequent itself contains concepts of the kind ordinarily called "empirical" (say, in the manner of Mill).

(2) *Obtaining protocol propositions relevant to the substantive hypothesis.* Judgments of observation alone neither confirm nor infirm substantive hypotheses. Such judgments must be first translated into factual judgments, whose terms are particulars, about objects, not simply about observations. These factual judgments are the criteria for the existence of the implicates of the hypothesis and hence of its truth.

Judgments of observation are always obtained with a view to this translation into relevant factual judgments. Before setting up an experiment we draw inferences from a substantive hypothesis which *might* be reached by translation of the possible judgment of observation. These inferences are in the *form* of factual judgments, though we do not yet know whether they are *actually* factual (true) or not. Then we try to obtain a protocol of observation which can be translated into this formally factual judgment.

There is thus a hypothetical movement underlying observation and its use. First, there is a hypothetical basis for seeking one kind of protocol instead of another. That is, the planning of an experiment or the search for a particular factual judgment is predicated on the substantive hypothesis.

Second, there is a hypothetical movement in the translation of the protocol into relevant factual judgments.

This second hypothetical movement is fairly complex. In the interpretation of a protocol proposition, there is, in addition to the substantive, another hypothesis. We shall call this the *operational hypothesis* because its antecedent is a condition subject to actual manipulation or operation. For instance, in deciding whether ethane consists of hydrogen and carbon, we appeal to a protocol of an analysis. This analysis and its interpretation are based on an operational hypothesis, viz., "If I analyze this sample of ethane, I shall obtain carbon and hydrogen." This is certainly suggested by the substantive hypothesis that ethane *does* consist of these elements. But the antecedent of the operational hypothesis may not obtain, as a matter of fact, in a particular case; and it may be that in some such cases—indeed, it is likely in most cases—this failure will bring about a failure of the consequent. Thus, for instance, if a mistake is made in analysis, carbon and hydrogen may not be obtained. But the evidential denial of this consequent does

not *necessarily* entail the falsity of the substantive hypothesis or its antecedent. This is true even in those cases where both hypotheses have formally the same consequent. Nevertheless, the operational hypothesis has one prerogative over the substantive, viz., the former can be directly confirmed by *modus ponens,* whereas the substantive hypothesis can be confirmed only by the seriatim infirmation of its alternatives. Thus if we have independent evidence for the antecedent of the operational hypothesis, we can use its actual consequent as independent evidence for or—more effectively—against the substantive hypothesis.

(3) *The regressive interpretation of the judgment of observation.* A scientific experiment is based on a complex hypothesis with both substantive and operational antecedents. The full hypothesis for our experiment is: "*If* ethane consists of hydrogen and carbon, *and if* I correctly analyze this sample of ethane, I shall obtain hydrogen and carbon."

If I do not actually obtain hydrogen and carbon, at least one of these antecedents must be denied. I may assert that the substantive hypothesis is false, or that the experiment actually tells me nothing because an error renders the operational antecedent false. I *can* stop the process of infirmation at the operational level if I choose. A teacher does so regularly when checking a student's laboratory notebook and finding experimental results inconsistent with accepted natural laws.

There are two procedures by which infirmation can be so localized. First, we may weigh the probability of the result's being accurate against that of the system as a whole containing an error. This involves the estimation of accidental error, or the proposal of *ad hoc* historical or psychological hypotheses which may keep the system as a whole more parsimonious than any attempted systematic revision.

Second, we may challenge the translation of the protocol into factual propositions. This translation always presupposes propositions within the system which are not at the moment being tested, but which are essential to the elimination of systematic error.

Which procedure we follow—the rejection of an operational hypothesis or the rejection of some collateral hypothesis within the system—will be decided in the light of the over-all economy of the system, i.e., in answer to the question, "Is the system more parsimonious and fruitful with *ad hoc* hypotheses at the operational level or with a revision of the corpus of substantive hypotheses?" No general rules can be given for a decision on this question.[2]

[2] This is argued in detail in my "The Principle of Parsimony in Empirical Science," this JOURNAL, Vol. XL (1943), pp. 617–633. Arthur Pap in his

The rejection of the operational antecedent is often only an evasion of an empirical test.[3] Now I assert that the first distinctive trait of an empirical method lies in the predominant weight given to the legitimacy of the *modus ponens* of an operational hypothesis. By virtue of this the infirmation is arrested at the operational level only when there is independent evidence to impugn the accuracy of the protocol (accidental error) or its translation (systematic error). That means that in an empirical method there is a tacit denial that there is any proposition in the corpus of a science that is immune to infirmation. A non-empirical method may be defined as one which tends to arrest infirmation at the operational level in order to exempt some substantive hypothesis from infirmation.

It now remains to apply this conception to an actual example. We wish to test Avogadro's hypothesis: "Equal volumes of gases under the same conditions of temperature and pressure contain equal numbers of molecules." This proposition is a long way from a protocol and is certainly not empirical in origin, in Mill's sense. To find whether it belongs to an empirical science we must first see if there is a methodological route which will lead from it to a proposition which can be derived from a protocol of observation. If so, we must elaborate the conjecture so as to infer a formally factual judgment, then obtain a protocol and translate it into this factual judgment or one logically relevant to it, and finally examine this judgment in its implications for the truth or falsity of the hypothesis.

We can not test Avogadro's hypothesis directly, since we can not count the molecules in a gas. There is, fortunately, an indirect route to testing.[4] The molecular theory implies that gas reactions

The A Priori in Physical Theory (New York, King's Crown Press, 1946), has more recently given a detailed account of the manner in which propositions empirical in origin come to be given a "functionally analytic" rôle and thus become *relatively* immune to infirmation.

[3] The rejection of the operational antecedent takes the form of an indication of either accidental or systematic errors. Quite in accord with the conclusion reached here, Dr. Pap says: "In general, if the accidental errors have been taken into account . . . and the law still fails to be verified, one will conjecture the presence of some cause of systematic error." Continuing along the line followed in this paper, he says, "The word 'conjecture' is essential: if the existence of systematic error were apodeictically inferred from the failure of a law to be confirmed by experimental data, then, obviously, no law could ever be refuted by experiment" (*op. cit.*, p. 97).

[4] The following account is not meant to be historically accurate. Avogadro formulated his hypothesis in 1811 in order to explain Gay-Lussac's law (1808) and some of the anomalies which led Dalton to reject Gay-Lussac's work. The important contribution of Avogadro was to show how Dalton's

occur between individual molecules. Assuming this to be true, we may predicate the following factual judgment on Avogadro's hypothesis: "One volume of hydrogen should combine with one volume of ethylene under identical conditions of temperature and pressure to yield one volume of ethane under the same conditions." This is implied, for if the numbers of the reacting molecules are in a 1:1 ratio, and if equal volumes contain equal numbers of molecules, the reacting volumes of the gases should also be in a 1:1 ratio. This formally factual judgment is testable by the following operational hypothesis: "If I combine one liter of hydrogen with one liter of ethylene at 50° C. and 760 mm. pressure I should obtain one liter of ethane under the same conditions."

The chemist prepares his gases in as pure a state as possible, measures out one liter of each of them under the stated conditions, mixes them to form twice the volume of the mixture containing, presumably, equal numbers of molecules of each, and then reacts them in the presence of a catalyst. He notices that both temperature and pressure change, and he runs a test, perhaps with a spectrograph, to determine when all the reacting gases have been used up, i.e., when there is no longer any free ethylene and free hydrogen in the chamber. When he is satisfied that the reaction is complete, he reads the manometer, thermometer, and barometer and writes his protocol. He then calculates what the volume of the new gas (ethane) would be under the initial conditions of temperature and pressure, basing his calculations on the well-known gas laws.

If the corrected volume is equal, within experimental limits, to that of the hydrogen or ethylene, he says Avogadro's hypothesis is confirmed. He does so because he can translate his protocol concerning pointer readings into propositions of the form, "The temperature is 51°, requiring such and such correction." This translation of protocol propositions into factual judgments asserting results which would be obtained under identical conditions is justified by appeal to the generalizations involved in the gas laws, the interpretation of the spectrogram, and the knowledge we have of the reaction of hydrogen with ethylene; likewise it assumes the repro-

laws were consistent with Gay-Lussac's observations if it were assumed that isolated elemental gases, as well as compound gases, consist of molecules instead of atoms, and that the equality of numbers held between molecules. But this historical circumstance is not of importance for our illustration, and the account given here is logically tenable, since (at least now) there is other evidence strongly suggesting Avogadro's conjecture. Let us imagine, then, that the hypothesis was formulated to take care of this other evidence, and then see how Gay-Lussac's experiment might have been used to test it.

ducibility of the results, i.e., the accuracy of the instruments and the competence of the operator.[5]

Let us suppose that the factual judgment so derived is *not* as predicted by Avogadro's hypothesis. Is the hypothesis to be excluded or revised? Not necessarily. We have three choices: we can assert that there is some error; we can deny or modify some previously accepted principle which dictated the choice of this particular experiment and underlay the translation of the protocol or, finally, we can deny Avogadro's hypothesis itself.

As to the first, it is quite obvious that infirmation should sometimes be arrested at the operational level, as in the case of a teacher correcting a laboratory notebook. But necessary as this is, it is nevertheless a departure from an unrestricted empiricism *within the procedure being examined,* and the only justification for it is independent evidence of error.[6] Without this other evidence, such infirmatory procedure is dogmatic, not empirical. The arrest of infirmation here might preserve a false system intact forever, for an operational antecedent always intervenes between a protocol and its systematic interpretation, and the denial of the operational antecedent can therefore serve as a kind of insulation between the body of a science and corrective contact with experience. It is a distinguishing feature of non-empirical procedures that when they do "appeal" to judgments of observation, they so stack the cards against the result that the threat of infirmation is restricted *a priori* to the operational hypothesis.[7]

[5] It is often said, and it was mentioned at the beginning of this essay, that a method consists in repeatable steps in a repeatable series. I have not taken this as definitive of empirical method, but it is easy to see how essential it is. We can not make generalizations or test them when made unless we suppose that the part of the hypothesis under our control, i.e., the operational antecedent, can be repeated without change with respect to different materials and in the light of different substantive and collateral hypotheses involved in its translation. This fact certainly plays a part in the apriorism of certain operationalistic theories.

[6] The "independent evidence" will be quite different in the two cases: for accidental error it will usually be a technological, historical, or psychological fact or plausible conjecture, as in the discovery of reaction times from astronomical measurements, and its categories will be distinct from those in terms of which the observation would have been described had it been accurate; evidence for the systematic error will be in terms indigenous to the field of description in use and can be eliminated conceptually by estimating the other factors or parameters, but until it is eliminated, it will seem to infirm the substantive hypothesis. It can be eliminated only on the assumption of the validity of the collateral hypotheses, one of which will be rejected if both the substantive hypothesis and the observations are insisted upon.

[7] *Cf.* the technique of those opponents of Galilei who argued that the craters of the moon were filled with an invisible substance.

But suppose that our devotion to empiricism prevents our stopping infirmation at this level except when there are compelling reasons, independent of a dogmatic assumption of our substantive hypothesis, for suspecting the accuracy of the observation and its report; then shall we reject the hypothesis?

This would be too simple. Though our purpose was to test Avogadro's hypothesis, our experiment might equally well have served to test other hypotheses (*collateral hypotheses*) presupposed in the actual experiment. A failure of this experiment could be interpreted as evidence against the law of definite proportions, the theory that gases consist of molecules, the chemical theory of the addition of hydrogen to compounds like ethylene, or the gas laws. If infirmation does not stop at the operational level, at least one collateral hypothesis or the substantive hypothesis must be rejected. The choice of the hypothesis to be considered infirmed will depend on the economy of the system as a whole. Scientific revolutions occur when someone decides that an experimental result does not infirm the hypothesis in question but some other assumption underlying the experiment.

If the first distinctive trait of an empirical method is refusal to arrest infirmation at the level of operations, the second comes to light here. It is the denial that there are any hypotheses in a system which are exempt from possible infirmation. Though at any time only one hypothesis is explicitly tested, there is no proposition in a science which may not be brought into question by reexamining the economy of the system in the light of the evidential denial of formally factual judgments predicted by the system.

It should be noted that these two traits are closely related, though in performance they may seem to play quite different rôles. Their similarity and difference are clearer if we reformulate them:

(1) No special class of propositions (e.g., so-called factual judgments) can be held *a priori* to be preëminently subject to error and hence to discount.

(2) No class of propositions can be held *a priori* to be uniquely free from the possibility of infirmation and discount.[8]

The corpus of a science consists of a hierarchy of propositions, some of which are central and *prima facie* essential to the economy of the system, while others are peripheral and perhaps even doubtful; but all are dubitable. The distinctive characteristic of an em-

[8] This is equivalent to Felix Kaufmann's "principle of permanent control." *Cf.* his "The Structure of Science," this JOURNAL, Vol. XXXVIII (1941), pp. 281–293, and "The Logical Rules of Scientific Procedure," *Philosophy and Phenomenological Research*, Vol. II (1942), pp. 457–471.

pirical method is not to be found in any requirement of psychology or epistemology that the propositions in the corpus be empirical in origin. What is required is (*a*) that any accepted proposition be subject to rejection when it conflicts with singular propositions derived from judgments of observation; and (*b*) that this privilege of exclusion be exercised against central as well as peripheral propositions, i.e., against hypotheses of high generality, definitions functioning *a priori,* or even against postulates, when rejecting an operational hypothesis can not be independently justified. Though we sometimes do and should test facts by theory, in truly empirical method the reverse takes precedence in critical cases.

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METAPHYSICS AS HYPOTHESIS[1]

I

ALL metaphysical theories should be regarded as hypotheses. The study of metaphysics may yield knowledge, but this knowledge is hypothetical in exactly the same way as are the theories of natural science. The hypotheses of natural science can not be proved with absolute certainty; neither can any doctrine of metaphysics.

This view of the nature of metaphysical knowledge is to be contrasted with both dogmatism and skepticism. Dogmatism assumes that it is the business of metaphysics to reach certainty of knowledge about the nature of things. It furthermore assumes that of two propositions or systems of propositions that are contradictory or contrary, only one can be true. Thus it follows that if one system of metaphysics is true, all others are false. This claim has been made for many different systems either by the authors or by enthusiastic disciples. Even if it be admitted by the dogmatist that his system is not invulnerable, nevertheless he can maintain his dogmatic position by holding that the trouble lies in inadequate details, and that if these could be eliminated or revised and the whole system readjusted, it would be true. Or, the dogmatist may hold that because of the limitations of the human mind it is impossible to know all the truth, but he maintains that he knows at least some metaphysical truths with certainty. Such

[1] Parts of this paper were read at the meeting of the Southwestern Philosophical Conference, Dallas, Texas, December 28, 1945, and parts at the meeting of the Southern Society for Philosophy and Psychology at Charlottesville, Virginia, April 20, 1946. The paper has been prepared in its present form with the aid of the Tulane University Council on Research.

an attitude is completely unjustified if metaphysical doctrines are hypotheses.

The view that metaphysics is hypothesis also disagrees with skepticism, for the latter holds that we do not have any metaphysical knowledge, whereas the former holds that we can. Both skepticism and dogmatism operate from the same initial assumptions. Both look for principles or materials from which can be obtained *certain* knowledge about the nature of things. The dogmatist thinks that he finds them, but the skeptic does not. Thus it has been usual in the history of philosophy for the person interested in metaphysics to be either a dogmatist or a skeptic, but this is quite unnecessary. The person who holds that metaphysics is hypothesis is neither. He says that the initial assumption of both dogmatist and skeptic that principles or materials of certainty are necessary is incorrect. He points out that natural science yields knowledge although its theories are only hypotheses: theories that can not be proved with certainty any more than can the doctrines of metaphysics. If the hypothetical results of metaphysical speculation are not worthy to be called knowledge, then it is possible to raise doubt on the same ground that scientific theories are so worthy. To maintain that scientific theories are not knowledge would be perilously near to maintaining that the term "conceptual knowledge" is without significance.

The subject-matters of metaphysics and of the natural sciences differ. Thus it is to be expected that the details of method in framing and verifying hypotheses will differ in the two fields, but the general meaning of hypothesis and verification will be the same. This view claims that the only proper method of philosophic study is the scientific method, thus reclaiming for philosophy what philosophy itself contributed to the world of intellectual endeavor.[2]

The hypothesis that metaphysical knowledge is hypothesis is not new, but it has never been fully investigated or exploited. In fact, it is only within comparatively recent times that the full import of the view that *natural science* yields only hypothetical knowledge has been realized. Admirable as Karl Pearson's *Grammar of Science* may have been, it lays little stress on the nature and importance of hypothesis.[3] A recent article, excellent in many

[2] I do not define scientific method only in terms of that application of it made in the natural sciences. For a more detailed treatment see my paper, "Scientific Method and Knowledge," in *Philosophy of Science*, Vol. 10, p. 67.

[3] See Third Edition (London, 1911) p. 87: "Scientific law is valid for all normal human beings, and is unchangeable so long as their perceptive faculties remain at the same stage of development." A scientific hypothesis is, for Pearson, only an untested law. See p. 100.

respects, by Mr. Conant, President of Harvard, recognizes the importance of hypothesis in natural science, yet does not recognize that an education in the "tactics and strategy of science" is primarily an education in the method of hypothesis.[4]

II

The full import of the view that metaphysical knowledge is hypothetical can not be realized until some of its interesting implications have been pointed out. In the first place, hypothesis and dogmatism are completely incompatible. Natural science found this out some time ago. If metaphysics yields hypothetical knowledge, we must give up our dogmatic pretensions or aspirations. Hence metaphysicians can no longer expend their energies on trying to prove or defend a system. To verify a hypothesis and to defend a system are two very different things.

A hypothesis, in science, is a generalization or series of generalizations such that, if it is assumed, the particulars of experience will fall under it as special cases and thus be explained. The logical form of a hypothesis is, "If the generalization *G* (law, theory, or such) is true, then the observed experiences *a*, *b*, and *c* are explained, in that they are cases of the generalization." When the verification of the hypothesis is in question this takes the form, "If the generalization *G* is true, then the special cases *a*, *b*, and *c* will take place." One then sets about to find out whether or not they do so. Scientific hypotheses are always general in nature, explanatory in intent, and never categorical. Hypothetical judgments neither assert by itself the antecedent, which is the general principle and is called the hypothesis both in science and in logic; nor do they assert by itself the consequent, which is made up of the particulars of experience or ultimately refers to them. All that the hypothetical judgment asserts is the *relation* between antecedent and consequent; but this assertion is of utmost importance, for it is in the asserted relation that the grounds of understanding lie—metaphysical understanding as well as any other kind. Metaphysical principles are the most general of all explanatory principles; and what the dogmatic method in philosophy does is to assert categorically the antecedent of a hypothetical judgment. This is a procedure without logical justification.

Another interesting implication of the view that metaphysics is hypothesis is that in that case there is no such thing as the Truth as metaphysicians have been wont to use the term. Metaphysics is not the search for Truth. Metaphysics is not even the search

[4] "The Scientific Education of the Layman," *Yale Review*, Autumn, 1946.

for the truth about reality. There is no such thing as the truth about reality. Reality is a category. One can make a hypothesis about reality, but he can not deduce the truth from a hypothesis and know that it is the truth. Further, it would seem logically that there is no such thing as Reality (at least with a capital R). Everything is real in some category and unreal in others. Morally real may not be physically real, and vice versa. There are as many kinds of reality as there are basic categories, and whatever can not be thought under some category can not be thought at all. If we have an experience that can not be placed under some category, we are unable because of that very fact to make any judgment whatever concerning its reality.

As the subject-matter of metaphysics is different from that of the natural sciences, the details of method in the framing and verifying of hypotheses will differ in the two fields. Logical analysis plays a larger part in the verification of metaphysical hypotheses. Scientific verifications always go back to particular observations or experiments. Observation can not be as determinant for metaphysics, however, for what is observed is a natural phenomenon. Natural phenomena are always subject to alternate interpretations, and metaphysics is interpretative. Metaphysics deals with the categorial activities of the mind.

The categorial activity and the understanding are very closely connected, as Kant maintained. In the last analysis, the understanding probably must be defined in terms of the categorial activity. Nevertheless, the place of the categories in metaphysics is different from their place in the natural sciences. Metaphysics criticizes its categories in a way that the natural sciences do not. This fact makes the verification of the hypotheses of metaphysics a much more complicated matter than the verification of the hypotheses of natural science. Metaphysics is both more interpretative and more critical than the natural sciences.

What is required beside logical consistency in a metaphysical hypothesis is adequacy in ordering the whole of our experiences.[5] This is the ruling principle of its categorial criticism. The verification of a metaphysical hypothesis requires that it be able to order experience so that we can "get around in it" both by action and by understanding. And it must be remembered that experiences of value can be left out no more than can the data of natural science. A metaphysics must furnish categories by means of which we can deal with all of our experiences; categories such that we can make judgments of real and unreal that will not bring

[5] "Nothing can justify a theory except its explaining observed facts," *Collected Papers of C. S. Peirce*, 1.170.

us either to grief in our practical affairs or to frustration in our intellectual enterprises.

III

In way of illustration, let me now consider three metaphysical problems. I propose that the metaphysical doctrines involved are and ought to be considered as hypotheses, without regard to whether they have been traditionally so considered. Mr. Pepper [6] has taken four great world systems as examples of metaphysical hypotheses, but I do not want to take my illustrations on the grand scale. Lesser metaphysical doctrines can be considered more or less on their own merits, although I would agree that in the final analysis their adequacy is to be judged in the light of their systematic implications. I shall briefly consider Cartesian dualism, the existence of other minds, and the existence of certain kinds of physical entities.

Cartesian dualism was one of the most widely prevailing metaphysical tenets of early modern times. E. T. Mitchell points out that this dualism was an important factor in setting up modern science.[7] He says,

> The problem of seventeenth century science was to distinguish what belonged to the physical system from what did not. . . . In isolating the physical system from the rest of nature, [the scientists] realized that colors, sounds, felt temperatures and the like were irrelevant to the dynamic relationships among bodies, but were relative to the sensitivity of the observer. These qualities had to be given some status, so they were located in the mind. Thus the physicists defined their field. . . . There was need of a separate science standing on its own bottom, that of psychology. Descartes gave the clearest definition of these two fields by making the physical sciences the study of the laws of extension based on material substance, and the mental sciences the study of the laws of thought based on spiritual substance. . . . Cartesian dualism, therefore, speculative metaphysics though it was, *made* modern science by clearing the way for quantitative physics and erecting psychology into an independent science.

The procedure Mr. Mitchell describes is surely an invalid one if the dualism of mind and matter is dogmatically conceived to be either true or false. It is not invalid, however, if the dualism be conceived as a hypothesis. The careful critic can not say that the Cartesian dualism has ever been demonstrated to be true. Nevertheless, its acceptance was useful. It furnished the early scientists categories that served well to order the phenomena with which they

[6] S. C. Pepper, *World Hypotheses*, University of California Press, Berkeley, 1942.

[7] "Metaphysics and Science," *Philosophy of Science*, Vol. 13 (1946), No. 4.

dealt. It enabled them to "find their way around" in their field of investigation. If we are to understand the place of Cartesian dualism in the history of thought, we must regard it as a hypothesis. It is a hypothesis that must be entertained whether accepted or rejected. Most of us may think today that the reasons for rejecting it are sufficient, but in the seventeenth century the reasons for accepting it may have seemed compelling.

Let me consider another metaphysical problem: that of the existence of other people's minds. Descartes' proof of other minds was not adequate. Berkeley had to have other minds to keep his system from falling apart, and he smuggled them in, but he ought to have *proved* their existence. The attempts to prove their existence in the nineteenth century led to the statement of solipsism. The nearest that can be achieved to proving it is the argument from analogy, but analogy never yields strict proof. If one can not accept the existence of other minds without being able to prove it, I do not see how he can escape being a solipsist or taking Hume's conclusion and becoming a skeptic.

If one regards solipsism and the existence of other people's minds as two rival metaphysical hypotheses, there can not be much debate concerning which ought to be accepted and which rejected. Solipsism must be *entertained* as a hypothesis. Its implications can not be investigated without entertaining it, but to *hold* it is a different matter. It does not produce the categories that enable us to understand experience. It is not a fruitful hypothesis. Hence, it is inadequate and ought to be rejected. The skeptical solution of the problem also should be rejected. That leaves us with the hypothesis of the existence of other people's minds as the relatively most adequate hypothesis, and we are justified in speaking of our knowledge of other people's minds.

The third metaphysical hypothesis that I want to take up relates to the existence of physical entities, but instead of considering ordinary physical bodies it will serve our purposes better to consider what D. L. Miller calls "interphenomena."[8] Although these are posited to exist as physical entities, by definition they can never be experienced as such. He gives as examples of these "interphenomena" electrons, the components of the atomic nucleus, electromagnetic fields, etc. These are not methodological constructs. The concepts are intended to refer to physical existents. That is, the interphenomenon has a locus in space and time, and has energy. Interphenomena are posited to fill the gaps between phenomena, and relate phenomena in an intelligible and in a causal way.

[8] "Metaphysics in Physics," *Philosophy of Science*, Vol. 13 (1946), No. 4.

Let me quote from Mr. Miller's paper. "Although sense objects in themselves do not require that we believe interphenomena to exist, to *account* for sense objects requires that belief. Without interphenomena, physics would be logically and physically incomplete." "Sense objects are separated spatially and temporally and in our experience of them. To deal with them effectively requires that this spatio-temporal gap be filled." "The gap can not be filled with experienced or even experienceable phenomena." "[Physicists] have filled it by positing interphenomena."

Mr. Miller goes on to ask why the physicists do not just leave the gaps there. "Why not say simply that certain experiences are followed by certain other experiences and let it go at that? . . . Because our system would break down in the sense that prediction would be impossible." At the risk of elaborating the obvious, let me give an illustration: Mr. Miller is saying that unless the physicists assumed the actual existence of electrons, protons, and neutrons, not only would the explosion of the atomic bomb be unintelligible, but there would never have been any discovery of atomic fission.

Can we know with absolute certainty that electrons, protons, and neutrons exist? Of course not. To posit them is a metaphysical as well as a physical hypothesis. As long as it is the most adequate hypothesis available, we may speak of the physicist's *knowledge* of electrons. If this is true about physical interphenomena, how much more is it true about ordinary physical bodies. A relatively adequate metaphysical hypothesis warrants our talking about our *knowledge* of the external world.

IV

Most kinds of traditional epistemology furnish a poor foundation for the formulation and verification of metaphysical doctrines as hypotheses. They have been constructed within a dogmatic scaffolding and attempt to offer a justification of dogmatic pretensions. If metaphysics is to be regarded as hypothesis, good epistemological reason must be presented for so regarding it.

Furthermore, a method of verification is necessary, for, as I have already pointed out, the verification of metaphysical hypotheses can not be accomplished in the same way as that of the hypotheses of the natural sciences. The epistemology must itself be presented as a hypothesis and tentative acceptance must go to that system proving to be relatively most adequate.

I would like to suggest that the epistemology offered by C. I. Lewis in *Mind and the World Order*[9] furnishes a foundation for

[9] Scribner, 1929.

considering and testing metaphysical doctrines as hypotheses. He points out that "the problem of metaphysics is the problem of the categories." [10] Understanding is achieved by subsuming the particulars of experience under suitable categories. This, of course, is not as simple as it sounds, for in so far as the particulars of experience are distinct and formulated, they are already subsumed under categories. It is the business of metaphysics to explicate the formulation, the structure, the application, and the criticism of the categories. The suitability of a categorial scheme can be determined only by its degree of success in enabling us to "get hold of" our experience—to grasp it and to find our way around in it.

A categorial scheme introduces order and law into our interpretation of experience, and, without these, experience would be unintelligible. Some categorial schemes are better than others, however, and the only choice between them rests upon the degree to which they do make experience intelligible—the degree to which they enable us to respond successfully to the given element in experience and to understand. Mr. Lewis points out exactly the way in which the understanding can not be separated from the response *when we are dealing with experience.* Metaphysics explicates and criticizes the categories of reality, and everything is real in some category and unreal in others. It is the business of metaphysics to get all this straightened out.

Mr. Lewis calls his view "conceptualistic pragmatism," [11] but not without some hesitation, for he recognizes that this expression is almost a contradiction in terms to the common or garden variety of pragmatism. It is based largely on a strict adherence to the methods of modern mathematics and modern logic, and has its origins more in the thought of C. S. Peirce than in that of James or Dewey. The pragmatic element lies in the view that the verification of any categorial scheme must come back to its success in ordering experience so that we can "get around in it" both by action and by understanding.

If philosophers in general and metaphysicians in particular would give up their dogmatic pretensions and would present their theories as hypotheses and offer them for verification and testing by others, philosophy could attain a level of coöperation and community of endeavor as high as is that attained in the natural sciences. The trouble now is that every philosopher thinks that he has to prove something, and that if he can not prove it, he is discredited. A natural scientist who approached his study in this

[10] *Op. cit.*, p. 10.
[11] *Ibid.*, p. xi.

frame of mind would be a poor scientist indeed, and the advance of natural science would be greatly hindered thereby. Before a scientist accepts a hypothesis, he tries in every way he knows to overthrow it, and coöperates with others in testing it. As a human being he may have a sentimental desire to see his hypothesis verified, but as a scientist he has rigorously schooled himself not to let this desire influence his thinking. Unless the philosopher can become as hardy, he does not deserve his high calling. As a result of its attitude toward hypotheses, natural science has become a community enterprise in trying to learn something instead of an eternal wrangle between persons each of whom is engaged primarily in trying to prove something. Philosophy may well profit from the example.

I have presented the hypothesis that all metaphysical doctrines are hypotheses. Mine is a methodological hypothesis, and whether it is a good one depends, as in the case of all others, on its logical analysis and on what can be done with it. It seems to me to be in harmony with all that modern logic has learned about the nature of postulate sets and proof in knowledge. I have tried to show, by means of a few examples, how metaphysical doctrines can validly be interpreted as hypotheses. How can we be absolutely certain that electrons and other minds exist? We can not and we do not have to be. Until better metaphysical hypotheses are proposed we can continue to talk about our *knowledge* of electrons and of other minds. If the results of the best available hypotheses can not be called knowledge, then we have no theoretic knowledge, either metaphysical or scientific, relating to empirical questions, and we had better stop talking about it.

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THE NATURE OF THE PHYSICAL OBJECT

OUR problem is to explain how we know the physical world and, consequently, what the nature of the physical must be if it can be known by our modern scientific approach to it.[1] We do not have the same kind of problem that the ancients had, for with the excep-

[1] A recent illuminating discussion related to the present task is "Secondary Quality," in this JOURNAL, Vol. XLIII (1946), pp. 599–610, by Lewis White Beck. I hope to place the whole problem of primary and secondary qualities in its broader, scientific setting and to show why the problem has taken on its modern form.

tion of Democritus[2] and his followers the Greek philosophers assumed that the sensible forms of objects, such as colors, sound, etc., pass over into the mind unaffected by the mind. Of course in both ancient and modern science it is assumed that the object of knowledge or the *what* we know when we know the physical world is that which exists independent of mind. But in contrast to Greek philosophers the modern scientist does not assume that our sense experiences without exception are straightforward results of the passage of the forms of nature over into the mind. Rather, in some cases at least, such as color experience, etc., a translation or transformation is involved, and the color, say, of an object does not pass over into the mind directly from the physical world.

The earliest modern scientists such as Galileo and Descartes were explicitly conscious of the necessary distinction in science between *primary* and *secondary* qualities. Galileo writes:

> I feel myself impelled by necessity, as soon as I conceive a piece of matter or corporal substance, of conceiving that in its own nature it is bounded and figured by such and such a figure, that in relation to others it is large or small, that it is in this or that place, in this or that time, that it is in motion or remains at rest, that it touches or does not touch another body, that it is single, few or many; in short by no imagination can a body be separated from such conditions. But that it must be white or red, bitter or sweet, sounding or mute, of a pleasant or unpleasant odour, I do not perceive my mind forced to acknowledge it accompanied by such conditions; so if the senses were not the escorts perhaps the reason or the imagination by itself would never have arrived at them. Hence I think that those tastes, odours, colours, etc., on the side of the object in which they seem to exist, are nothing else but mere names, but hold their residence solely in the sensitive body; so that if the animal were removed, every such quality would be abolished and annihilated.[3]

In a very similar vein Descartes separates sense experiences from the nature of the physical body.

> In this way we shall ascertain that the nature of matter or of body in its universal aspect, does not consist in its being hard, or heavy, or coloured, or one that affects our senses in some other way, but solely in the fact that it is a substance extended in length, breadth and depth.[4]

Although it is true that these early modern scientists furnished the mathematical and rational basis for breaking away from medieval science, nevertheless their view is not consistent with the lab-

[2] Democritus describes both atoms and all collocations of atoms in pure geometric terms. For him sensations are perturbations of the soul which itself consists of fire atoms. Consequently it can not be said of Democritus that he believed colors, odors, etc., pass over into the mind unaffected by the mind. "By convention sweet is sweet, by convention bitter is bitter. . . ."

[3] Dampier-Whetham, *A History of Science*, pp. 146–147.

[4] Descartes, *Selections*, edited by Ralph M. Eaton, p. 291.

oratory method which is absolutely essential to the development of modern science and to the way we learn about the objective, factual world. A justification of the laboratory procedure necessarily leads to the conclusion that sense experience, as over against pure reason, is indispensable to our knowledge of the physical world and, consequently, to our knowledge of the physical object. Since this is the case our fundamental question is: By what sense or senses do we learn about the physical object and what is the nature of the physical object?

Our task is to show how in modern science we have separated that which belongs to the physical world as such independent of mind from that which mind usually confers upon the world but which does not constitute a part of the physical object.

It should be noted that in contrast to Newtonian physics Descartes conceived of matter (the corporeal or physical) in terms of pure extension. Apparently his belief that the world is a plenum is not the result of his inability to conceive of empty space (contrary to Aristotle). Rather Descartes' plenum concept is a result of his unwillingness and inability, from purely rational principles, to state the nature of the physical object in experiental terms. In support of "The Great Mr. Newton," Locke's contention that *solidity* is a primary quality of matter is wholly unacceptable to a Cartesian or to any thoroughgoing rationalist. If solidity, as over against pure extension, is a primary quality of matter differentiating empty space from occupied space, and if through the sense of touch we experience solidity, then at last we have discovered the experiential basis for the concept of the physical object, and the modern thesis that the laboratory method leads us to a knowledge of the physical world may be justifiable.

Certainly Locke's statement that *solidity* is a primary quality does not solve the mystery of how through experience we can know that which we can not experience, namely, the physical object. Locke's explanation is inadequate mainly for two reasons. First, the sensation of solidity is passive as is the Lockian "idea" that follows from it, for the "idea" of solidity in Locke's system is due to the passage of the sensation into the mind unaffected by the mind. A consequence of this doctrine would be that lower animals "know" the physical object in the same way and to the same extent as the best scientist. Second, Locke can not explain how we can conceive of the *insides* of physical objects, for the inside of an object can not be touched nor reached through actually dividing the object into parts since all actual division gives new surfaces and new outsides, but no inside. An answer to Locke's inadequate statement requires showing how in behavior (in dynamic relation-

ships—interaction of our bodies with other bodies) the concept of the physical object with an inside emerges.

Only in contact experience (the "pushiness" experience—Whitehead; or the pressure experience) is empathy possible. Colors are in no sense equal to the responses they call out in us,[5] nor are sounds, odors, or tastes. But the physical object stimulates us to act toward it in the same way it acts toward us. In acting on a physical object action and reaction are the same,[6] even as they are among insensitive physical objects. From the standpoint of sensations as sensations in the Lockian, passive sense, all are alike. The pressure sensation is no different from that of color, sound, etc., when we consider it in its purely passive form, as all early British empiricists did. But when considered in its dynamic relation to the sensitive organism, i.e., considered from the standpoint of the responses it elicits in us, the pressure sensation is fundamental in the sense that it is that with reference to which all others are oriented. It is always the touchable object that is there as a necessary condition for normal sensations of color, sound, odor, and taste. It is the touchable object that is manipulated in controlling the order and nature of all sensations. The locus of control of all sensations is in this touchable, *physical* object. The nature of the physical object in its native form consists in mass. As a physical object it is not colored, nor does it have odor, taste, or sound. It is that which offers resistance to having its momentum changed. It is the inert.

The *meaning* of a physical object for a person is not to be found in one's memory. Nor is it in the immediate experience of pressing against that object. Rather, its *meaning* has a temporal dimension which can be stated in terms of *inhibited responses* only. For the lower animal there are simply responses to and in that animal's environment, but no consciously inhibited responses. Inhibitory responses require a highly developed cerebrum and possibly a language, for an inhibited response is the representation to one's self of an overt act, although the act is not carried out in fact. An

[5] See *The Philosophy of the Present*, by G. H. Mead, p. 123. I am indebted to both Mead's lectures and his written works for suggestions of the nature of the physical object. I believe what Mead said on this subject is true, but his statements are often cryptic and disconnected from their larger scientific setting.

[6] Here we may consider Newton's special contribution to the clarification of the concept of mass. Newton's third law of motion, To every action there is an equal and oppositely directed reaction, implies that the physical action by us on a body is the same (numerically—in terms of force) as its action on us. An experience of this interaction is the psychological basis for the concept of the physical object.

inhibited response is, as Mead expresses it, a collapsed act. A thing is to us a physical object only in so far as our responses to it are inhibited. We become conscious of the insides of objects (i.e., we know them as physical objects) through these inhibited responses. The object is a physical object to us in so far as we are conscious of what in a future could be done to it. Here I am trying to show that the meaning of the physical object can not be given in static terms such as images, Lockian ideas, or direct, immediate impressions. Nor is there significance in asking: what is the essence of the physical object? as if we can give a purely formal definition of it free from all experience and interaction with it.[7]

Inhibited responses are confined to the physical object. One can not have an inhibited response to colors, odors, etc. A color may be remembered and anticipated, and it may be a sign of what particular experience is to follow, but we can not have an inhibited "color" response. We can respond to the colored *object*, and if that response is inhibited, the object is a physical object to us. The locus of the meaning of the physical object is found in inhibited responses.

All "distant" experiences, such as colors, sounds, and odors, become significant in relation to the touchable, physical object only. For this reason they are secondary; i.e., they are signs (Berkeley) of experiences to follow, including finally the contact experience and the manipulatory, resistant object. Furthermore, the spatio-temporal order of distant experiences[8] can be controlled only in so far as we can *react with* the physical object which for these reasons is said to be the physical condition (or cause) of distant experiences. In so far as we conceive of ourselves as agents controlling the order of events and thereby our experiences of these events, we say we *act on* the physical object. If indeed we had no control of the order of events, then all sense experiences, both the distant and the immediate contact experiences, would be on the same methodological level, for none of them would be said to be the condition for any other and at best they would be mere signs of each other in Berkeleian fashion. Hence in men-

[7] See Lewis White Beck, *loc. cit.*, pp. 603 ff.

[8] From the standpoint of control the physical object has priority over all others and, consequently, the touchable object is of primary importance in modern science. There may continue to be arguments over whether the world is "spiritual or physical" etc., and we may go so far as to consider the untouchable electromagnetic waves as constituting the "real" world. But so long as there is any control of events the touchable, massive body is central. To control color experiences, we must work on material bodies. The same can be said of all secondary experiences.

tioning "primary" and "secondary" qualities, we are not supposing that some are metaphysically more real than others. In fact according to my thesis it is meaningless to ask whether the primary qualities are metaphysically more real than the secondary qualities. Psychologically they are all on the same level. Methodologically the pressure experience is primary in that it is the experience by which we know that which enables us as scientists to control the spatio-temporal order of other experiences.

The discovery of the meaning and nature of the physical object separates modern science from ancient and medieval science, for it is through this discovery that men have a knowledge of the objective, physical conditions for control. Upon this discovery men began to disabuse their minds of the belief that we are spectators whose scientific business it is to find and to know the essence of things for the sake of knowing, but that we can not control events through natural means. With the passage of this idea we see also the disappearance of the significance of the question: What is matter as over against mind? Descartes' complete separation of the two, as we have suggested previously, was not strictly conducive to our contention that through experience—a dynamic interacting experience—with *mass* we arrive at the concept of the physical object. Newton, I believe, gave an adequate scientific statement of the concept of the physical object, but of course he did not explain how, through experience, we arrive at that concept. He left the epistemological gap unclosed. This gap can be closed only by an explanation of the meaning of the physical object in terms of the functional relationship between the minded biological organism, which is itself also a physical object, and the physical world.

Two points need further discussion. First, it may not be sufficiently clear that there is a difference between *having* the direct pressure experience with the physical object on the one hand and the *meaning* of "physical object" on the other hand. Second, to some it may not be clear that mass, which in the manipulatory area is invariant, is that which in experience must be referred to as the physical object when objects are stripped of all secondary qualities. Let us discuss the *meaning* of the physical object first.

One must arrive at the meaning of a physical object by taking its attitude.[9] By this we mean that one must respond covertly—or be conscious of a prepotent response—toward and with an object. In doing this one has the meaning of the physical object. This implies, of course, that the meaning has a temporal, futuristic di-

[9] G. H. Mead, *op. cit.*, p. 122.

mension (based on past experience). The meaning of the object is what can be done to and with it in a future. But that is something that is not now being done to or with it. Its meaning, then, is what we are not doing to or with the physical object, and what we may never do to or with it, but which could be done to or with it *according to our physical theory*. The meaning requires an abstraction. This abstraction consists in having a covert response to the process of interacting with an object, and that process, of course, requires time. When such an abstraction takes place we have the meaning of a physical object, and when this meaning is put in its generalized form we have the concept, "physical object." That abstraction requires the highly developed cerebrum by means of which the organism can present to itself the overt act, but in so presenting the overt act the spatio-temporal element is shortened, thereby giving us, as Mead says, the collapsed act. This abstracting process by which we conceive of the physical object is quite different from the mere sensation of the inert object.

Our response to different kinds of objects is different and, therefore, different objects have different meanings. E.g., our response to a delicate glass figurine is different from our response to a baseball. These objects have different characteristics, and these characteristics are physical in so far as they are expressible by physical laws, such as the laws of elasticity, momentum, etc. In handling the figurine we treat it "with care" because of what could be done to it. These conscious inhibited responses are what the figurine means to us as a physical object. Of course the figurine may be a social object or an esthetic object, but our responses to it as such objects do not constitute any part of its meaning as a physical object. Its social, esthetic, etc., characters can not be stated by use of physical laws and are not, therefore, physical. Our discussion of the nature and meaning of the *physical* object presupposed this qualitative, irreducible difference between the physical object and other types of objects.

The *meaning* of a particular object is not identical with that object. Meanings, or inhibited responses, condition our actual response to the object, and if our actual responses so conditioned do not lead to the anticipated results, the object may be reinterpreted. That is, it may take on new meanings. If upon accidentally dropping the figurine it does not break, our inhibited responses are changed and the object takes on a different meaning. Meanings, therefore, can be tested for their adequacy or inadequacy.

In our consideration of mass as that which is experienced by the sense of touch and pressure, we have not overlooked the consequence of the theory of relativity, namely, that the mass of a body

increases with its velocity. Our experience of the physical object or its mass is always in a particular locus confined to the manipulatory area of the individual. It is not the case that our pressure experience in a different space-time system would be different. Rather in a manipulatory area, regardless of the consentient set of events [10] in which we find ourselves, our pressure experiences are the same with the same object. The pressure experience, then, remains fundamental.

Transformation formulae are designed to calculate experiences we or others would have in other than the system from which our calculations emanate. These calculated or predicted experiences are, however, of the same type as those in our present environment and all of them refer finally to the manipulable, touchable object. The transformation formulae are, finally, methods of going from one perspective (of the physical object) to another, and their significance depends on the immediate experiences we have in acting with or on the physical object. These immediate experiences are contact, pressure experiences.

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BOOK NOTES

Platon. Sa Conception du Kosmos. HUGO PERLS. New York: Éditions de la Maison Française, Inc. 1945. 2 volumes: 310 pp., 252 pp. $7.50.

The purpose of the book is stated in the preface. Written in the library of a friend, a French writer, who had lost his life in one of the resistance groups during the war, "*le livre, dû à l'angoisse des mois interminables après la bataille perdue, accomplirait sa plus belle tâche, s'il contribuerait tant soit peu à la realisation d'un mot de Platon: 'Si tu pouvais convaincre tout le monde, Socrate, il y aurait plus de paix et moins d'injustice.'*" Those who are not "convinced" may at least share some of the author's "consolations of philosophy" in this calm and comparatively detached study of Plato's cosmology.

While M. Perls' method of approach is quite evidently Kantian, he is not unaware of the failures of previous attempts to assimilate the Doctrine of Ideas to the *Critique of Pure Reason.* He avoids the pitfalls to which such eminent commentators as Natorp, Lutoslawski, and J. A. Stewart had exposed themselves in assuming that a knowledge of modern idealism *via* Kant is indispensable to an

[10] See Whitehead, *The Principles of Natural Knowledge,* pp. 31–32.

understanding of Plato. "Plato must be understood by himself," says M. Perls, with commendable insistence, although his advice that questions of the authenticity of dialogues and differences in their interpretation should be settled only on the basis of *"la pensée platonicienne même"* may seem a counsel of perfection. It may be true, though difficult to prove, that Plato's cosmology was more "critical," in the strictly Kantian sense, than Kant himself and most of his followers had been willing to admit, and that the "Copernican revolution" in philosophy was more Platonic in its origins. Equally difficult to prove would be the contention, which M. Perls is fond of repeating, that a knowledge of Plato from the original text, instead of being derived from a second-hand version from the school of Leibniz and Wolff, would have caused Kant to change his views on several important points.

What British empiricism and Leibnizian rationalism were to Kant, the skepticism of the Sophists and the dogmatism of the Eleatics were to Plato. He transformed the simple Kosmos of Ionian speculations into a systematic "order of thought," existing apart from the visible world, which nevertheless "participates" in it. This *"ordre criticiste"* is organized as is the work of an artist, or the soul itself, in accordance with its own regulative ideas. The problems of "separation" and "participation" are discussed in the three lengthy sections of Chapter V. The necessary separation of the ideas from the sensible phenomena is inferred mainly from "the elective affinity" of concepts, all honorific when pertaining to "the invisible order of the soul," wherein alone there is freedom of the will, and all the reverse when pertaining to the causally determined phenomena. The necessary participation of the phenomena in the ideas—the necessity of a "mixture"—is inferred from the methodological requirements in Plato's dialectic (and "the philosophy of Plato is nothing but a methodology," I, p. 147). Systemic distinction of parts presupposes "an anterior methodological composition." In the third section of the chapter, the idea of not-being is "deduced" for the "construction" of experience, which is regarded as a participation and mixture.

The nature of opinion is related to an analysis of its conditions (interval, space, time, causality, and Eros) and to the several functions of the Ideas, including (besides that of *doxa*) the functions of hypothesis, paradigm, and finality. Antinomies arise in the "topology" that is involved in Plato's scale of values, and they are resolved in his conception of an ideal and rational immortality.

In Volume II, a theory of esthetics is explained on the basis of the previous analysis of dialectic and its presupposed "separation" and "participation." The esthetic judgment, which is teleological,

as Kant had maintained, employs its own "unique idea of beauty" to determine whether the artist's "imitation," in a particular work of art, does or does not participate in the idea. "Esthetics and the fine arts are separated from nature, from the body, and from everything that resembles the body: from all matter (*de la matière entière*). The separation of science, knowledge, and mathematics is deliberate. By means of these distinctions, the esthetic judgment and artistic production are freed from numerous motives and rules which, by and large, would jeopardize their liberty" (II, pp. 85–86). Not even the artist himself is competent to judge the value of his work without training in the Platonic dialectic (II, pp. 120–123). Likewise, the moral judgment and morality are freed from capricious motives and arbitrary rules. Dialectic makes possible an ordered freedom, inspired from above. It brings to an end the vain search for a mechanical and psychological freedom which is only the arbitrary freedom of the will. Plato had realized what Kant had been unable to discover in the whole history of philosophy.

M. Perls has fittingly dedicated his book to the memory of the neo-Kantian, Hermann Cohen.

D. S. M.

Critical Thinking. An Introduction to Logic and Scientific Method. MAX BLACK. New York: Prentice-Hall, Inc. 1946. xv + 402 pp. $5.00 and $3.75.

"Logic," says the author of this new text, "ought to be easy, interesting, and enjoyable." And he has succeeded on all three counts. Logic, of course, ought to be some other things as well, and on these there may be some doubts concerning the author's treatment.

The text covers most of the field usually included in an elementary course: deductive reasoning, truth-tables, disjunctive arguments, the syllogism, a study of language (meaning, definition, fallacies, etc.), and scientific method. There are numerous very rich examples that do not have the flavor of the re-cooked meal one usually finds. Indeed, some of the examples tax the ingenuity of the student (and, alas, the instructor) far beyond the explanation in the text.

It is to be regretted that the treatment of methods in the sciences is presented in examples that so often replace a careful exposition better designed to enlighten the student. Also, there will be a definite handicap for a student who is to learn something about modern methods in science, to be taught the method of concomitant variations without an introduction into the notion of corre-

lation. At least within our present university curricula, logic courses are the only places the science student gets a chance to see the general aspects of the methods he will have to use. He therefore suffers a considerable loss of education if he does not then become acquainted with the modern ways of making scientific inferences, ways that go far beyond the canons of induction, interesting as these are from an historical point of view.

Perhaps the weakest point of the text is the author's own analyses of practical problem-situations. So many of the very excellent examples he gives depend for their proper solution on a far more complex psychological and sociological background than is provided, so that the suggested proper reasoning appears decidedly *ad hoc* (e.g., in the "hint" for the solution of the export-import problem in No. 2 in the Exercises in Reading on page 10).

It is unfortunate that the syllogism is still being handled in a non-logical fashion. The rule, for example, that "two negative premises yield no valid conclusion" (p. 126) has only accidental validity relative to the four categorical forms traditionally discussed; it does not have the formal validity of the rule that two affirmative premises do not imply a negative. Yet the two rules are here presented to the student in the same manner, and the student comes to feel that both are essential laws of a general logic.

These are all special points of comment. The text should certainly be seriously considered for introductory logic courses. Its scope, carefulness, and richness of material at least qualify it that far. The text is best suited for logic courses in which scientific (experimental) method is of secondary, and logical forms are of primary, importance.

C. W. C.

Thomas Jefferson: Champion of Religious Freedom; Advocate of Christian Morals. HENRY WILDER FOOTE. Boston: The Beacon Press. 1947. ix + 70 pp. $1.25.

In ten exceedingly brief, charmingly-written essays Mr. Foote presents an account of Jefferson's religious views and his relationship to Unitarianism. Mr. Foote is concerned to show from Jefferson's own writings that the charges of atheism and immorality leveled against Jefferson in his own day were unfounded. He does not consider that in those days any deviation from Trinitarian belief was regarded as atheism. The sketchiness of Mr. Foote's essays was most regrettable in his discussion of the Act for Establishing Religious Freedom in Virginia, and in his suggestion (very tentatively offered, it is only just to say) that William Ellery Chan-

ning was directly if unconsciously influenced by Jefferson's views during his two years as tutor in the Randolph family. Mr. Foote has, however, brought together some interesting materials in highly readable form.

J. L. B.

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LA PENSÉE. No. 10. Karl Marx et la pensée moderne: *A. Cornu.* Courte histoire de l'existentialisme. III: *Henri Mougin.* L'attitude rationaliste: *René Maublanc.*

We have received the following reprints:

"Aristotle's Conception of Language and the Arts of Language" by Richard McKeon. From *Classical Philology,* Vol. XLI (1946), pp. 193–206, and Vol. XLII (1947), pp. 21–50.

"A Theory concerning Free Creation in the Inventive Arts" by Harry B. Lee. From *Psychiatry,* Volume III (1940), pp. 229–293.

"A Critique of the Theory of Sublimation" by Harry B. Lee. From *Psychiatry,* Vol. II (1939), pp. 239–270.

"The Dance Motive of the Latin Dance of Death" by Ernst Moritz Manasse. From *Medievalia et Humanistica,* Number 4, 1946.

"Economic and Social Thought of Gerrard Winstanley. Was He a Seventeenth-Century Marxist?" by Winthrop S. Hudson. From the *Journal of Modern History,* Volume XVIII (1946), pp. 1–21.